

June 7, 1956

Notre cher Alain:

This letter is something of an act of contrition. Your christmas card has been resting in the "urgent" file of correspondence, as a constant reproach; the false urgency of hard-to-mouth business is all that has kept us from answering. Actually, we have not been altogether remiss: last year as soon as you mentioned that you were interested in receiving the Scientific American, I started what proved to be involved negotiations with the publishers: the trouble was that I asked one of my colleagues here who already receives the magazine to put you down for a gift subscription, as this would save you the magnificent sum of \$1; unfortunately, their bookkeeping machinery seems to have broken down over this arrangement, and it was many months before they acknowledged the order. They finally did so not long ago, and claim that you have been receiving the journal; I hope this is correct. (The annual subscription is \$6.50 under these conditions; you must still have some \$\$ to your credit: I hope you have a record of how much, and I hope your future instructions will be more promptly executed. Have you had any further difficulty with the Society of American Immunologists?)

Your colleagues' innumerable visits to the U.S. have given opportunities for the exchange of cordialities, but I am very sorry we have not been able to see one another for so long. Jacques again teased us about "when we could come to Europe"; no one seems to believe us when we say it is a simple matter of finances, and that Europeans have a far easier time finding subsidies for transatlantic visits than we do. But it is so. Jacques proved himself again the most charming of egoists during this last trip (last November), but I am afraid that the momentary perfection of each transient scheme of enzyme synthesis evokes some impatience after a while: his account this time of the "y" system seemed to many to be glib, beautiful and unsatisfying. But we have to withhold judgment until the full experimental details are published so they can be critically reviewed.

Jacques and we had an illuminating discussion about motivation in science; I wonder if you can tell me if this is typical of the French outlook. I had remarked that there was no point getting excited about the scientific fads of each moment, except insofar as they gave an immediate personal enjoyment. From a global, historical perspective, lasting importance would attach not to the obvious progress of each moment, but to completely new ways of looking at scientific problems that ipso facto will not be appreciated by contemporary thought; we would have no way of predicting what would be the real advances of our science; at most the most brilliant among us would constitute a footnote, not a chapter in the ultimate history of science. Jacques' answer was that he was interested only in absolute, ultimate 'truth', and that if he did not believe his work was in the forefront of historical advance, if he could not believe himself to be so-to-speak a Pasteur, he would abandon science. I am not sure how far this reflects his actual viewpoint, and how much this was just taking the other standpoint for argument's sake.

During the last year, not much altogether new has happened in the lab, and as usual much of our work is in the progression of earlier findings. I am sure our papers are a better way of telling you about it. Esther is still trying to trace

the prophage through the Gal-lambda transduction system; Kalckar (at Bethesda) has lately been doing the enzymology of the Gal⁻ mutants. Curiously enough, the most common mutant has the same enzymatic block as is found in the human genetic disease of galactosemia. More surprisingly, some of the mutants have blocks in different steps, and we are now trying to see if the biochemical and genetic organization of the Gal⁻ mutants shows any correspondence. Somewhat to my surprise, there are hints of a story similar to Demerec's, but the work hasn't gone far enough to warrant any conclusions. I myself have been occupied with writing up old work (e.g. a terrible job now in organizing collected pedigrees on the hereditary "chains" of motility in abortive transduction in Salmonella, ~~xxx~~ parallel with Bruce Stoeckert's work) and with conjugation processes involving various Hfr strains (a variety that Elie and Francois had discovered about the same time I had stumbled onto it). I am also trying some DNA-transduction experiments with bacterial protoplasts, so far with no/ luck at all.

Last November, we had to move out of our lab. so that it could be remodelled; we moved back only in April. We are now much more comfortably set up, though space is still not very large; except for occasional difficulties with storage, however, we are quite comfortable and have room for 8-10 people doing our kind of microbiological work. Part of our difficulty comes from having to be self-contained as we are the only microbiologists on campus. You probably won't remember most of the group now here, ~~xxxxxxxxxxxxxxxxxxxx~~ as the others have all gone elsewhere; I won't burden you with their present identities— there are three students, one US, one Australian, one Japanese, and all excellent, and three postdoctorals besides Esther and me. One of these you might know, Newton Morton, who did his thesis with Jim Crow in statistical-human genetics. He is joining the medical faculty to initiate a program in genetics there, but wants some laboratory experience first. Next year will be quite a potpourri: a Danish couple (Ørskov); a German (Heumann) and a Finn (Saris) are coming; it strikes me that relatively very few French students and postdoctorals have come to the students— is this only because Paris already has so many opportunities in the fields I know about, or is this also true of your science courses at the universities?

How about your own work? I know only that you had gotten interested in "acquired tolerance"— what about it? Of course, this subject might have the most fascinating genetic implications, as the mechanism might involve the transduction of some hereditary entities (chromosomal, cytoplasmic?) from the graft to the host cells. What are your ideas as to its mechanism? I hope you are not neglecting any opportunity to use exact genetic differences, specifically Snell's isogenic-resistant mouse strains, for this kind of work. An interesting angle on this subject, which I have been waiting to see for a long time, has appeared recently: a paper by Good of Minnesota (in an out-of-the-way medical journal called "Journal-Lancet", June '55) describes experiments with agammaglobulinemic children. These children tolerated grafts of lymph nodes from normal individuals, and thereby became able to produce antibodies. An adequate means of suppressing the existing antibody-forming mechanism would ~~xxx~~ seem to be the sine qua non for development of tolerance in adults. (I am sure you will also have seen the note in Nature recently by Ford et al on rat-mouse chimeras). However, I just had a letter from Snell wherein he concludes that his enhancing factor system is based on the production of just enough antibody to suppress further sensitization of the host by the graft; obviously, there has not been enough application of "classical" serological technique in the study of these phenomena; I assume that circulating antibody is not demonstrable in prenatally acquired tolerance. Is this so?

I hope our atrocious performance as correspondents does not discourage you, any more than it is a reflection of our hope of preserving a warm amitié.

With the best from both of us,

